

Interactive comment on “Distribution and behaviour of dissolved selenium in tropical peatland-draining rivers and estuaries of Malaysia” by Yan Chang et al.

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The manuscript of Chang et al presents a combination of field study and laboratory experiments. This work reports first analysis of the distribution and behavior of dissolved Se in several rivers and estuaries of western Borneo. The concentration and speciation of Se were assessed using state of the art analytical techniques. The authors also evaluated the fate of Se species in estuaries and characterized the DOSe in rivers and estuaries. Synthetic figure 9 is especially appreciated. The origins of DISe and DOSe seem to be correctly identified. Specific comments to consider for improvement of ms: L74-87: This is too detailed literature review, not directly linked to the subject of this

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study. It is probably not necessary. Physio-geographical parameters of rivers should be listed in a table (% of coverage by peat, degree of affection by palm plantations, runoff, slope etc) Three type of Se behavior are well identified and summarized in L 349-358. However, the presentation of each individual river in Figs 2-5 takes too much space. Either consider presenting just an example of each group or the average of all rivers in each group L407-412: Please explain, what is the mechanism of Se(IV)/Se(VI) increase with DO increase. Oxidation is more pronounced at high DO, yet the observations are reverse to that. L434-439: This information should be in the site description table of river watershed parameters L468-477: There are certainly some structural data (e.g., XAS) on molecular status of Se bound to organic matter. L505-507: The analogy with NO₃ is not straightforward: nitrate is a nutrient but Se is not always a nutrient Fig 8a-c present the data from other papers and as such not necessary. Citations of main results from these papers would be enough. L580-643: Basically, the same comment. Too many specific details from other papers; the whole section can be greatly shortened, and only main findings are presented. L621-625. This conclusion is true, however it is based on very indirect observations (many parameters are from already published works). Note that the main source of Se in peatland waters as from highly aromatic DOM of peat horizons has been recently evidenced in Siberian lakes (Pokrovsky et al., 2018 Env Sci Technol) Fig 2: How representative is Rajang to other rivers, why it is shown? Fig 3 is fine Fig 4 might not be needed - may be in Supplement? Previous Fig 3 is way more informative. Fig.4 should be shortened, at least. Fig. 6: what is the difference with fig 3? (hard to apprehend) Fig 6: The size of panels is too small, please enlarge Fig. 8: The plots showing no relationships between variables are not needed to be shown; it is enough just to state that there is no link between variables. General comment: The authors could present the fluxes of Se to the ocean, in different forms. The yield from watersheds of different rivers (i.e., in kg/km²/y) could be compared with that of other large and small rivers of the world, if the data are available. How important are small rivers of Borneo on a global scale of DISe and Dose delivery to the ocean? Are the yields disproportionately high? Conclusions nicely reflect

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the main findings, and even if some of them are speculative (L 675-678), they can be stated as they are. Oleg S Pokrovsky

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